

# Occam's razor meets WMAP

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Using a variety of quantitative implementations of Occam's razor we examine the low quadrupole, the "axis of evil" effect and other detections recently made appealing to the excellent WMAP data. We find that some razors *fully* demolish the much lauded claims for departures from scale-invariance. They all reduce to pathetic levels the evidence for a low quadrupole (or any other low  $\ell$  cut-off), both in the first and third year WMAP releases. The "axis of evil" effect is the only anomaly examined here that survives the humiliations of Occam's razor, and even then in the category of "strong" rather than "decisive" evidence. Statistical considerations aside, differences between the various renditions of the datasets remain worrying.

## I. INTRODUCTION

A better fit to the data can always be obtained by appealing to a theory containing more free parameters. The extra knobs can't harm, and quite often help the job of fitting data. Intellectual honesty, however, tells us that a better fit may then not signal evidence for the theory, but merely unfair advantage over its competitors. Confronted with two theories fitting the data equally well we'd prefer the simpler one, the theory containing fewer parameters or based on a less complicated model.

Such considerations form the basis of Occam's razor, but a quantitative formulation is notoriously hard to come by. It's clear that the real "evidence" should combine the naive goodness of fit with a penalty function measuring the complexity of the theory. But several distinct rationales for doing this may be found in the literature, notably the Akaike [1] and Bayesian [2] information criteria (AIC and BIC) and the Turing machine based criterion proposed by one of us [3]. Simplicity, it seems, is in the eye of the beholder.

Furthermore, subjective double standards seep into the analysis, and the rigors of penalization are often reserved to results one doesn't like. For example, the CMB community has resisted applying Occam's razor to inflationary parameters (see [4, 5, 6] for notable exceptions) and to some power spectrum features [9, 10]; but with reference to anomalies unpalatable to just about everyone (such as the "axis of evil" effect, the embarrassing statistical anisotropy exhibited on the largest angular scales [11, 12, 13]), the strictest penalization is enforced [15]. (The criterion employed therein to scrutinize the axis of evil effect is loosely the AIC.)

We applaud this type of application of Occam's razor, but we believe it should be employed impartially. The purpose of the present paper is to examine some of the proposed Occam razors, and to apply them democratically to both "likable" and "undesirable" features in the large-angle CMB anisotropy. We examine the WMAP data [15, 16, 17], in its first and third year releases, and in various renditions dealing differently with the galactic

plane. We focus on claims for departures from scale invariance and for reionization (Section II), the evidence for a low quadrupole and a low  $\ell$  power cut-off (Section III), and the strength of the detection of the so called axis of evil effect (Section IV).

## II. BRANDS OF OCCAM'S RAZOR

We first review some well-known criteria for evidence, adopting a notation similar to that of [3]. Let  $\mathcal{L}$  be the likelihood and  $k$  the number of parameters of the model. They will be tuned so as to maximize the likelihood or, equivalently, minimize the information  $I$ . The information in the data given the theory is defined as minus the logarithm of the likelihood. But in fact we want to minimize the information in the data *and* the theory together, that is:

$$I(D, T) = I(D|T) + I(T) \quad (1)$$

so that  $I(T)$  is the penalty referred to above.

According to some authorities, strong evidence for a theory over a "base model" requires an improvement in  $I(D, T)$  by at least 3 (see [7, 8]). The title of "decisive evidence" is not normally bestowed unless the improvement exceeds 5.

All the razors we will wield fit into the above scheme, but they differ in how they define  $I(T)$ . According to the Akaike information criterion (AIC) the information in a theory is simply its *number* of parameters, so that

$$I_A(D, T) = -\ln \mathcal{L} + k. \quad (2)$$

This is obtained by an approximate minimization of the Kullback-Liebler information entropy.

Rather different is the the Bayesian information criterion (BIC), based on the penalty

$$I_B(T) = \frac{k}{2} \ln N \quad (3)$$

where  $N$  is the number of data points being fit. It results from an approximation to the true Bayesian evidence,

giving the model a uniform prior. The full Bayesian evidence, where one integrates the likelihood over the full set of parameters, has also been considered.

The criterion developed in [3] interprets  $I(T)$  entropically and algorithmically. It estimates the information in a theory  $T$  in terms of the number of bits of “memory” required to *store* the parameters. From this point of view, a theory’s complexity depends not only on how many parameters it contains, but also on the precision with which they are stored. The resulting penalty term  $I(T)$  is not simply a function of  $N$  and  $k$  but depends on the details of the theory. Typically it includes a term equal to (3) as well as other terms that ensure that a theory with more parameters than data points will never be judged a good fit. The advantage this approach has over AIC and BIC is that it never has to appeal to the asymptotic approximation  $N \gg 1$ . It’s disadvantage is that it is harder to apply since  $I(T)$  is not a simple universal function of  $N$  and  $k$ . In the spirit of the abbreviations “AIC” and “BIC” we will refer to this third razor as “HIC” or simply “ $H$ ”, since the differential goodness of fit is denoted by  $H$  in [3].

The various criteria do not always agree, even qualitatively. Take the statements that WMAP displays strong evidence for reionization and a spectrum of density fluctuation that is not scale invariant [15]. Both assertions rely on an improvement to the fit  $\Delta I(D|T) \approx \Delta\chi^2/2 = -4$  (see Table 3 in [15]) and for both, this costs an extra parameter. Using AIC we get  $\Delta I(D, T) = -3$  pointing toward a detection. But  $N$  is between 1500 and 3100, so  $\ln N$  is around 8. Using BIC this implies  $\Delta I(D, T) \approx 0$ , most definitely not a detection. We have not worked out the HIC value of  $I$ , but we would expect it to resemble BIC more than AIC, leading again to the verdict of “no detection”. Of course one cannot drop both departures from scale invariance and reionization and there are strong astrophysical reasons for preferring reionization to a tilted spectrum. Therefore based on WMAP it seems prudent to say that there is no strong evidence for  $n_S \neq 1$ .

### III. IS THE QUADRUPOLE UNDERPOWERED?

Much attention has been paid to the low power observed in the lowest multipoles ( $\ell = 2$  in particular), but how strong is the evidence when shaved with Occam’s razor? This is essentially a problem of variance estimation. Given a sample and an externally inferred variance  $\sigma_E^2$ , when is it worth revising  $\sigma_E^2$  in the light of the sample? Here  $\sigma_E^2$  is obtained by appealing to a theory of the whole spectrum, dependent only on a small number of parameters (e.g.  $\Omega$  and  $\Omega_\Lambda$ ). These are fixed primarily by the higher multipoles (the Doppler peaks), so as far as the low multipoles are concerned  $\sigma_E^2$  is external.

The “null hypothesis”  $H_0$  is that  $\sigma_E^2$  is correct, and the observed low power a fluke. Since the costs of estimating  $\sigma_E^2$  are borne elsewhere,  $I(T) = 0$  and  $I(D, T) = I(D|T)$ .

The catch is that the fit to the data is far from perfect. Introducing the “observed variance” of the sample,

$$\sigma_S^2 = \frac{1}{N} \sum x_i^2 \quad (4)$$

we have

$$I(D, T) = -\ln P(D|T) = \frac{N}{2} \left[ \ln \sigma_E^2 + \frac{\sigma_S^2}{\sigma_E^2} \right] \quad (5)$$

far from its minimum.

The alternative hypothesis  $H_1$  is that the power is indeed low and that  $\sigma_E^2$  should be replaced by an internal estimate,  $\sigma_I^2$ , obtained using the sample and bearing its costs. The procedure for applying HIC can be adapted from [3] and goes as follows (the only novelty is that here the average is known). Firstly, we minimize  $I(D|T)$ , with solution  $\sigma_I^2 = \sigma_S^2$ . This cannot be stored to infinite accuracy, so we expand around the minimum:

$$I(D|\sigma_S, \Delta\sigma) = \frac{N}{2} [\ln \sigma_S^2 + 1] + N \left( \frac{\Delta\sigma}{\sigma_S} \right)^2. \quad (6)$$

Averaging over a uniform distribution in  $\Delta\sigma \in (-\delta\sigma/2, \delta\sigma/2)$  gives  $\langle \Delta\sigma^2 \rangle = \delta\sigma^2/12$ , so that:

$$I(D|\sigma_S, \delta\sigma) = \frac{N}{2} [\ln \sigma_S^2 + 1] + \frac{N}{12} \left( \frac{\delta\sigma}{\sigma_S} \right)^2. \quad (7)$$

The storage penalty, on the other hand, is

$$I(T) = -\ln \frac{\delta\sigma}{\sigma_S} \quad (8)$$

so  $I(D, T)$  is minimized for optimal accuracy:

$$\delta\sigma = \sqrt{\frac{6}{N}} \sigma_S. \quad (9)$$

Thus the information in the data and  $H_1$  is

$$I(D, T) = \frac{N}{2} [\ln \sigma_S^2 + 1] + \frac{1}{2} - \ln \sqrt{\frac{6}{N}} \quad (10)$$

The evidence  $H$  against the null hypothesis  $H_0$  is the difference between its information and that in  $H_1$  (positive  $H$  favors  $H_1$ ). This may be written as  $H = H_f - H_p$ , where  $H_f$  is the improvement in the fit

$$H_f = \frac{N}{2} \left[ \ln \frac{\sigma_E^2}{\sigma_S^2} + \frac{\sigma_S^2}{\sigma_E^2} - 1 \right] \quad (11)$$

(this is often approximated by  $-\Delta\chi^2/2$ ), and  $H_p$ , the penalty paid by  $H_1$  for introducing a new parameter, is

$$H_p = \frac{1}{2} \left[ 1 + \ln \frac{N}{6} \right] \approx \frac{1}{2} \ln N - 0.4 \quad (12)$$

An exact rendition of this argument (not appealing to Taylor expansion (6)) leads to penalty

$$H_p = \frac{1}{2} \left[ \ln \frac{N-1}{6} + N \ln \frac{N}{N-1} \right] + \psi(N) \quad (13)$$

TABLE I: Evidence for a low quadrupole, based on various datasets and Occam’s razors  $H$ , AIC and BIC.

Map	$H_f$	$H$	$H^{AIC}$	$H^{BIC}$
ILC1	2.47	2.11	1.47	1.67
TOH	2.62	2.26	1.62	1.81
DILC	2.08	1.72	1.08	1.27
WMAP3	2.32	1.96	1.32	1.51

where  $\psi(N)$  is a small negative correction, monotonic in  $N$ , that never exceeds 0.2 in magnitude and is totally negligible for  $N > 5$  (for example  $\psi(10) = -0.03$ ). The AIC would instead quote  $H_p^{AIC} = 1$  (with  $H^{AIC} = H_f - H_p^{AIC}$ ), whereas the BIC would introduce:

$$H_p^{BIC} = \frac{1}{2} \ln N \quad (14)$$

(with  $H^{BIC} = H_f - H_p^{BIC}$ ) which in the large  $N$  limit is the same as (12) (or (13)) plus constant 0.4. Generalization for many independent parameters is straightforward.

In Table I we examine the evidence for a low quadrupole. We consider the first year data as in [17] (ILC1) and in [18] (TOH), as well as the third year release [16], both the debiased internal linear combination map (DILC) and the MLE estimate (WMAP3). Clearly under Occam’s razor we can never claim a significant detection, whatever the dataset. Adding the octupole and other low  $\ell$  does little to improve the situation. Visual inspection of the plots in [16] shows that many of these low  $\ell$  “anomalies” have disappeared in the three year data. But they were never significant, as the analysis of the first year data presented in Table II shows. Naturally  $H_f$  improves as more and more multipoles are considered, but these bring in new parameters and so the associated “detections” are erased under the weight of Occam’s razor. This table refers to first year TOH; in other datasets/renditions the evidence is even lower. By bringing more  $\ell$ s into the analysis the evidence decreases further.

None of this will surprise several authors [18, 19, 20, 21, 22]; yet, to drive the point home we stress that the evidence for a low quadrupole – bad as it is – is still stronger than the evidence for a non scale invariant spectrum under the BIC. Also the message has yet to fully filter to enthusiastic theorists (e.g. [9]). For example claims have been made [24] that DGP gravity [23] fits better the low  $\ell$  spectrum. While it might be true that the theory achieves a better fit without introducing new parameters (and therefore doesn’t fall prey of *further* penalties) the fact remains that it corrects a misfit that is not significant to begin with.

#### IV. THE AXIS OF EVIL

Many paths lead to the axis of evil. Planarity statistics [11], Maxwell multipole vectors [12, 25], and  $m$ -

TABLE II: Evidence (or lack thereof) for low power at small  $\ell$  using the most sympathetic dataset (TOH).

$\ell$ or $\ell$ range	$H_f$	$H$	$H^{AIC}$	$H^{BIC}$
2	2.62	2.26	1.62	1.81
3	0.35	-0.18	-0.65	-0.62
2-3	2.98	2.08	0.97	1.19
4	1.18	0.51	0.18	0.09
2-4	4.16	2.59	1.15	1.28

TABLE III: The planarity of the  $\ell = 2, 3$  modes using TOH (top rows) and WMAP3 (bottom). There is nothing anomalous with the planarity of  $\ell = 2$  and  $\ell = 3$ , taken on their own. It’s the fact that the planarity occurs in roughly the same direction (and with roughly the same suppression ratio  $\epsilon$ ) for both multipoles that substantiates the anomaly.

Data	$\ell$ s	$(b \quad l)$	$\epsilon$	$H_f$	$H^{AIC}$	$H^{BIC}$
TOH	2	58 -103	.030	3.09	0.09	0.68
	3	62 -121	.025	5.06	2.06	2.14
	2-3	61 -113	.032	7.48	4.48	3.76
WMAP3	2	70 -127	.036	2.84	-0.16	0.43
	3	62 -122	.035	4.29	1.29	1.37
	2-3	64 -123	.038	6.89	3.89	3.16

preference statistics [13] are examples. Here we focus on the planarity of  $\ell = 2, 3$ , that is, the fact that in the frame pointing to  $(b, l) \approx (60, -100)$  in Galactic coordinates, the power is concentrated in the  $m = \pm \ell$  modes. How seriously should we take this?

The more abstract estimation problem is: when is it justified splitting the  $a_{\ell m}$  sample into sub-samples with different variances? This is a variation on the calculation in the previous section with a subtlety: the result is frame dependent. Consider a sample with  $N$  elements and sample variance  $\sigma_S^2$  (the  $2\ell + 1$  modes of a multipole), and two sub-samples with  $N_1$  and  $N_2$  elements and sample variance  $\sigma_{S1}^2$  and  $\sigma_{S2}^2$  (the planar modes  $m = \pm \ell$ , and all the others). The difference in  $I(D|T)$  between the null hypothesis (don’t split the sample) and the alternative hypothesis (split) is

$$H_f = \ln \frac{\sigma_S^N}{\sigma_{S1}^{N_1} \sigma_{S2}^{N_2}} \quad (15)$$

where  $N = N_1 + N_2$  and  $N\sigma_S^2 = N_1\sigma_{S1}^2 + N_2\sigma_{S2}^2$ . This depends only on the suppression ratio

$$\epsilon = \frac{\sigma_{S2}^2}{\sigma_{S1}^2} \quad (16)$$

and therefore one can consider the issue of planarity even if the evidence for an internal  $\sigma_S^2$  is small or nonexistent. The value of  $\epsilon$  depends on the  $z$ -axis coordinates  $(b, l)$ , which should be chosen to maximize  $H_f$ . In the process we add two more parameters to the Occam’s razor bill.

This is the procedure adopted for analyzing each multipole independently and in Table III we present results

for two datasets: TOH and the WMAP three year data. We find that  $H_f$  is around 3 for  $\ell = 2$  and 5 for  $\ell = 3$ , at the cost of introducing 3 parameters (the axis and the ratio of power  $\epsilon$ ) for each multipole. Using AIC this degrades  $H_f$  to a  $H$  around 0 and 2, respectively. Results for the BIC are reported in the same table. As in previous studies [11, 13] we find no serious evidence for an anomaly if *each multipole is taken on its own*. Given a random, statistically isotropic multipole there is always a frame in which most of the power is concentrated in a single  $m$ ; that this  $m$  equals  $\ell$  is not unlikely for small  $\ell$ .

What turns the axis of evil into a menace is that the maximal  $H_f$  for  $\ell = 2$  and  $\ell = 3$  is reached with roughly the same parameters (see values in Table III). Thus if we take a single axis and  $\epsilon$  chosen so as to maximize the total  $H_{Tf} = H_{Qf} + H_{Of}$ , we obtain a  $H_{Tf}$  only slightly worse than the sum of the separate optimal  $H_{Qf}$  and  $H_{Of}$ ; the parameter cost, however, is halved. Our results are described in Table III. The search for the joint axis was done numerically, and we see that the result is heavily weighed by the octupole. The common  $\epsilon$  was found via the method of Lagrange multipliers, i.e. by maximizing

$$H_{Tf} = H_{Qf} + H_{Of} - \lambda[\sigma_{Q1}^2\sigma_{O2}^2 - \sigma_{O1}^2\sigma_{Q2}^2] \quad (17)$$

with solution:

$$\begin{aligned} \sigma_{Qi}^2 &= \frac{\sigma_{SQi}^2}{1 \pm A/2} \\ \sigma_{Oi}^2 &= \frac{\sigma_{SOi}^2}{1 \mp A/2} \end{aligned}$$

where  $i = 1, 2$  indexes the sub-samples and  $A$  is the solution of a quadratic equation expressing  $\epsilon_O = \epsilon_Q$  (an equation that only depends on the sample ratio  $\epsilon_{SO}/\epsilon_{SQ}$ ).

As shown in Table III our evidence for an anomaly is always above  $H = 3$ , i.e. “strong evidence”. One may therefore wonder where is the discrepancy with the analysis in [15]? In that work the axis of evil was modeled as a modulation by an underlying large-scale function, and a model was found with  $H_f = 4$  (a chi-squared improvement of 8) at a cost of 8 parameters. Using either AIC or BIC the value of  $H$  is therefore negligible. However, here we exhibited a model improving the fit by about  $H_f = 7$  at a cost of 3 parameters. This (phenomenological) model is simply based on a diagonal covariant matrix for  $\ell = 2, 3$  of the form:

$$\langle |a_{\ell m}|^2 \rangle(\mathbf{n}) = c_\ell(\delta_{\ell|m|} + \epsilon(1 - \delta_{\ell|m|})) \quad (18)$$

Hence the poor evidence reported in [15] is not a deficiency of the axis of evil effect or the data, but merely a shortcoming of the proposed model itself. One can always find a model for any anomaly containing a number of parameters so large as to drive  $H$  down to a small value. But the issue is: what is the value of  $H$  for the best model of that anomaly, the model with the optimal trade off between fit and number of parameters? We have gone a fair way toward answering this question.

## V. CONCLUSIONS

In this paper we subjected to some of Occam’s razors three patterns that people have claimed to see in the CMB data: departures from scale invariance, a low quadrupole, and the anisotropy that has come to be known as the “axis of evil”. Specifically, we considered the razors that we called AIC, BIC and HIC. All three agreed to discount the claim for a low quadrupole, while in contrast, the two that we brought to bear on the axis of evil both suggested that it should be taken seriously. Only in relation to scale-invariance was there disagreement, with AIC tending to accept the claim and BIC definitely rejecting it. (We did not consult HIC in connection with the first and third effects, but we plan to do so in a later version of this preprint.)

It is somewhat embarrassing that Occam razors can disagree, but a glance at equations (2) and (3) reveals that this is inevitable, since the penalty terms  $N$  and  $\ln \sqrt{N}$  are very different when the number of data points  $N$  is  $\gg 1$ . By comparing these two expressions, one sees that BIC will be more lenient than AIC when  $N$  is small, but much tougher when  $N$  is big (the crossover coming around  $N = 7$ ). For HIC, it is harder to make a blanket statement, but experience has shown that it tends to agree more closely with BIC, probably since each relies, in its own way, on a version of Bayes’ rule.

In the case of the claimed departure from scale-invariance, we would thus expect HIC to agree with BIC in favoring a negative verdict, which at the very least should be added as a word of caution to the conclusions reported in [15]. By way of comparison, it’s worth pointing out that, even if we accept the more favorable value of  $H$  coming from AIC, the evidence for scale non-invariance is no better than that for the “axis of evil”. When all razors agree on a lack of evidence, as is the case with the underpowered quadrupole, one should definitely not lose sleep over the anomaly, and we hope keen theorists will divert their creativity elsewhere.

But even when different razors agree on an anomaly – such as the axis of evil – one should not trust the result blindly. The issue of systematics remains of paramount importance, as shown by the significant differences in  $H$  obtained from the various datasets and methodologies used to deal with the galactic foregrounds. And one should bear in mind that even the most enthusiastic “Ockhamist” would be unlikely to claim for his or her favorite razor a freedom from ambiguity [26] better than  $\Delta H = \pm 0.3$  or so. In addition it’s probably fair to say that the trouble of rewriting cosmology textbooks deserves in itself a penalty factor. This is hard to evaluate but it may translate into the requirement of a higher level of evidence than “strong”, at the phenomenological level. Perhaps the ever improving polarization maps will have a say on the matter and tilt the scales. This issue is currently being very actively investigated.

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